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METHODOLOGICAL STRATEGIES FOR EDUCATIONAL CHANGE.

BY- GUBA, EGON G.

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THIS STUDY OF STRATEGIES FOR EDUCATIONAL CHANGE CONCLUDES THAT THE AEXPERIMENTAL, OBSERVATIONAL, OR FIELD STUDY APPROACH IS PREFERRED TO THE EXPERIMENTAL OR LABORATORY APPROACH, BOTH FOR CHANGE RESEARCH, WHICH IS CONCERNED WITH THE ENTIRE PROCESS OF CHANGE, AND FOR EVALUATION, WHICH IS CONCERNED WITH THE ASSESSMENT OF SINGLE PHASES OF CHANGE. EXPERIMENTAL STRATEGY INQUIRES INTO POSSIBILITIES, WHEREAS AEXPERIMENTAL STRATEGY INQUIRES INTO ACTUALITIES. OTHER DISTINCTIONS INCLUDE SETTING, LEVEL OF CONTROL, SCOPE, NUMBER OF VARIABLES, TREATMENT, AND CONTEXT. EDUCATIONAL CHANGE IS A PROCESS INVOLVING FOUR STAGES, EACH WITH A PARTICULAR OBJECTIVE--(1) RESEARCH IS RESTRICTED TO THE ADVANCEMENT OF BASIC KNOWLEDGE, (2) DEVELOPMENT FORMULATES AN INVENTION OR SOLUTION TO AN ACTION PROBLEM AND PREPARES THE DESIGN FOR ITS INSTITUTIONAL USE, (3) DIFFUSION DISSEMINATES INFORMATION ABOUT THE INVENTION AND DEMONSTRATES ITS UTILITY, AND (4) ADOPTION ESTABLISHES AN INVENTION AS PART OF AN ONGOING PROGRAM. A NUMBER OF FIELD STUDY TECHNIQUES ARE DESCRIBED--(1) A SYSTEMATIZED DEVELOPMENT OF OBJECTIVES, DEFINED AS A PROGRAMMATIC APPROACH, WITH AN ORDERED ACTION TAXONOMY TO DEFICT, RELATE, CONCEPTUALIZE, AND TEST, (2) THE CONDUCT OF INQUIRY WITHIN AN EXPLICIT THEORETICAL FRAMEWORK, (3) A PHENOMENOLOGICAL IDENTIFICATION OF THE INVESTIGATOR WITH THE ACTUAL FIELD OF HIS STUDY RATHER THAN WITH AN EXPERIMENTAL CONDITION, (4) REPLICATION AND RECYCLING OF INQUIRY, DEFINED AS THE TACTIC OF ACCUMULATIVE EVIDENCE, AND (5) THE ANALYSIS OF PATHOLOGIES TO GAIN INSIGHTS INTO NATURAL SITUATIONS. THIS PAPER WAS PRESENTED TO THE CONFERENCE ON STRATEGIES FOR EDUCATIONAL CHANGE (WASHINGTON, D.C, NOVEMBER 8-10, 1965).

METHODOLOGICAL STRATEGIES FOR EDUCATIONAL CHANGE

Egon G. Guba
Assistant Director
School of Education
The Ohio State University

Paper presented to the Conference on
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It seems to me that there are two general strategies available to an investigator who seeks to inquire into a given set of phenomena, which I shall characterize as essentially experimental, manipulative, and interventionist, on the one hand, and aexperimental, observational, and laissez faire, on the other. Hard sciences such as physics and chemistry are typically experimental, although not always so, as is illustrated by the case of astronomy. Social sciences have usually attempted to use the former experimental strategy, perhaps in emulation of the modal physical science practice; this tendency is especially noticeable in experimental psychology. Many social science inquiries have relied on the latter aexperimental strategy, however, since, as in the case of astronomy, the phenomena under study simply do not lend themselves to experimental or manipulative approaches. It shall be my major thesis in this paper that the aexperimental strategy rather than the experimental strategy is particularly suited to inquiries in the area of educational change. To make this point I shall first attempt to delineate the two available strategies more closely. I shall then propose a conceptual paradigm for thinking about the change process, and, within the framework of this paradigm, give the reasons why the aexperimental strategy is to be preferred. I shall close the paper with a series of illustrations of tactics which might be used in pursuing the aexperimental strategy in order to build some conviction that is operational and empirically meaningful.

Major Differences Between Experimental and Aexperimental Inquiry

As Roger Barker notes in his presentation on ecological psychology which appears in a recent number of the American Psychologist,[1] empirical phenomena occur without benefit of intervention by an investigator. Data relating to those phenomena, however, are always the product of some kind of interaction between the phenomena and the investigator. Barker suggests that two kinds of interaction are possible, which I will equate, possibly erroneously, with the two general strategies to which I alluded a moment ago. In the first case, which I have labeled "aexperimental" and which Barker calls "Type T," the relationship between phenomena and data is transitive, i.e., the data simply pass through the investigator whose only contribution consists of certain transformations which he makes to render the data into coded form, so that they may be analyzed at the investigator's convenience at some other time. In the second case, which I have labeled "experimental" and which Barker calls "Type O," the relationship is looping, in that the investigator becomes an integral part of the data through intervention. T systems produce "data" which denote a world the /investigator/ did not make in any respect; they signal behavior and its conditions, in situ." On the other hand, the crucial feature of /O systems/ is that by becoming involved as an operator in the units he is investigating, the /investigator/ achieves control which allows him to focus upon segments and processes of

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[1] Roger G. Barker, "Explorations in Ecological Psychology," American Psychologist, 20 (1965), 1-14.

particular concern to him, via data that refer to events which he, in part, contrives. The essence of I systems is that they are natural and uncontrived, while the essence of O systems is that they are controlled and manipulated.

To illustrate the difference between these two modes, Barker gives an example of frustration research in children which I should like to quote. He says:

> Some years ago, when I was a student of Kurt Lewin, he and Tamara Dembo and I carried out some experiments on frustration. The findings of these experiments have been verified by others and they have become a part of the literature of scientific psychology. The experiments provided basic information about the consequences for children of frustrations, as defined in the experiments, and about the processes that produce these consequences. Time passed. In due course I had a student, and he undertook to study frustration. So far, so good. All in the grand tradition! My student, Clifford L. Fawl, did not replicate the earlier study; he did not contrive frustration for his subjects; he pioneered, and extended the investigation from children in vitro, so to speak, to children in situ. He searched our specimen records of children's everyday behavior for instances of this allegedly important phenomenon without psychologists as operators. Here are the words of his report:
>
> > The results. . . were surprising in two respects. First, even with a liberal interpretation of frustration fewer incidents were detected than we expected. . . . Second, . . . meaningful relationships could not be found between frustration . . . and consequent behavior such as . . . regression . . . and other theoretically meaningful behavioral manifestations.
>
> In other words, frustration was rare in the children's days, and when it did occur it did not have the behavioral consequences observed in the laboratory. It appears that the earlier experiments simulated frustration very well as we defined it and prescribed it for our subjects (in accordance with our theories); but the experiments did not simulate frustrations as life prescribes it for our children.[2]

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[2] Ibid., p. 5.

If we were to take a naive point of view with respect to this example of Barker's, we might pose the question, "Which of the two sets are the 'real' data?" But to argue for one over the other would be fruitless, since obviously both provide certain kinds of information. The choice between the two modes that any particular investigator might make depends, I will assert, largely upon his intent. The intent of the experimental investigator, I suggest, is one of inquiring into possibilities, while the intent of the aexperimental investigator is to inquire into actualities.[3] The basic question of the experimentalist is, "What would happen if . . .," while the basic question of the aexperimentalist is, "What does happen in the real world?" Thus the experimentalist selects on an a priori basis the variables which he wishes to relate, and then arranges a controlled situation so that the effects of other variables are eliminated or at least randomized. Under these conditions he can investigate various aspects of the selected variables regardless of whether such aspects or levels are ever likely to be found in nature. The aexperimentalist, on the other hand, may be unsure of the variables that are relevant to the problem of interest to him, or, even given that he can identify the crucial variables, is not interested in studying them in any form except as they do in fact occur naturally. This distinction between investigating possibilities vs. actualities is a crucial one that must be borne in mind in any discussion of inquiry.

Generally speaking, our habits of scientific thought predispose us heavily to the experimental mode. We are accustomed to thinking that

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[3] I am indebted to Dr. Lawrence Schlesinger of George Washington University for drawing my attention to this distinction.

experimental control is absolutely essential if unconfounded data are to emerge. In part we feel this way because we have come to take the physical sciences as our model of scientific inquiry. We have also tended to accept as the ideal social science model the methods of psychology, which are also heavily experimental. This ingrained point of view causes us to ask, "Why be satisfied with less then the most rigorously defined and controlled data generating systems?" Why, asks Barker, should we bother with "the motley class of methods variously called field methods, naturalistic approaches, observational techniques," when whatever it is that they do, laboratory experiments can do it with greater precision?

The answer to this question of course is that these two methods by no means produce comparable data. Aexperimental methods are not simply poor patched-up imitations of "real research " Each method complements the other, and does things which the other cannot do Experimental methods cannot provide data unaltered by the system generating them: they are inevitably tinged by a <u>laboratory bias</u>. Aexperimental methods, on the other hand, cannot focus unequivocally upon particular variables which interest the investigator nor trace clear cause-effect relations between them; such investigations are inevitably cast within a complex and unresolved situational matrix and yield data of an essentially probabalistic nature

There are other points of difference than the intent of the investigator between experimental and aexperimental methods which deserve attention, and I will list several of these briefly

1. <u>Setting</u>. Generally speaking, experimental inquiries are pursued <u>in vitro</u>, i.e., in contrived laboratory settings, while aexperimental inquiries are pursued <u>in situ</u> i e wherever they happen to be found.

2. Level of control. Experimental settings require that conditions of control be established over all variables. Possible confounding variables (i.e., all variables other than those of interest) are either deliberately controlled, as through a matching process, or randomized. The randomization process, indeed, is the basis for the generation of an error estimate which is the criterion by which the significance of other data are judged. Aexperimental settings not only tend to militate against the establishment of similar controls by their very nature, but indeed, in many instances, the intent of the investigator is such that conditions of invited interference[4] are deliberately sought. In this way the investigator can gain insights into what happens in the worst of all possible worlds, rather than in the best.

3. Scope. Because experimental inquiry is deliberately focussed, its scope may be described as molecular, while the scope of aexperimental inquiry is broader and may be described as molar.

4. Number of variables. Experimental inquiry is concerned with a sharply limited number of variables (even in so-called multivariate designs), while aexperimental inquiry must necessarily involve any variables which nature sees fit to intrude upon it.

5. Treatments. The term treatments is used here in the conventional research sense as a combination of variable conditions to which the subjects will be exposed. In experimental inquiry, the treatments are carefully

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[4] I am indebted to C. Ray Carpenter for this phrase.

designed beforehand, and one of the major purposes of the imposed controls is to ensure that treatment conditions vary as little as possible throughout the experiment. Such variation is seen as leading to an undesirable inflation of the error estimate. In aexperimental inquiry, no such conditions can be imposed, and the treatments are likely to vary throughout the inquiry. Thus a new science teaching method being field tested will be subjected to almost continuous variation as teachers become more familiar with it, as their favorable or unfavorable attitudes toward it become firmer, or as the materials themselves are changed through extension and refinement. If experimental treatments are fixed, aexperimental treatments are highly variable, but to the aexperimentalist, this fact is itself grist for the mill and deserving of study.

6. Context. Experimental inquiries are, by virtue of the controls imposed, made essentially free of the context in which they are carried out; indeed, this lack of context is the major virtue of the laboratory. Aexperimental inquiries, on the other hand, are highly contextual in nature, the particular outcomes of any aexperimental inquiry depending heavily upon the context in which the inquiry was carried out.

The preceding discussion differentiating experimental from aexperimental inquiry is summarized in Table I, for ease in subsequent referencing (see page 8).

The Nature of Educational Change

Having thus delineated the nature of the basic strategies of inquiry which are available to an investigator, I shall take it as my problem to make a determination of which strategy has the most immediate utility for

TABLE I

ESSENTIAL POINTS OF DIFFERENCE BETWEEN EXPERIMENTAL AND AEXPERIMENTAL INQUIRY

Area of Difference	Experimental Inquiry	Aexperimental Inquiry
Intent of the Investigator	Possibilities	Actualities
Setting	Laboratory	Nature
Level of Control	Absolute*	Invited Interference
Scope	Molecular	Molar
Number of Variables	Limited	Gross
Treatments	Stable	Variable
Context	Unrelated	Highly Relevant

*This condition cannot be met in many experimental inquiries for various reasons, e.g., inadequate resources or lack of access to an appropriate sample. In such cases "quasi-experimental" designs are often advocated "in tandem" so that deficiencies produced by lack of a certain kind of control in one design may be overcome by corresponding strengths in another design.

students of educational change. I do not mean to imply by this statement that one of these strategies is always preferable to the other, or that any determination of preference which we may make at this historical time will always hold. Our immediate problem, however, is how to get on with the study of educational change now, with whatever level of sophistication that we may possess and in relation to any questions that interest us currently. I cannot proceed to this next step with adopting

for analytic purposes some conceptual paradigm of the nature of educational change, and I will therefore make a brief excursion to describe such a paradigm which I have found useful, and which was formulated in another context by my colleague, David L. Clark, and me.[5]

We see the process of educational change as involving four stages (although not necessarily in a particular order, nor always with all stages being involved); research, development, diffusion, and adoption. This continuum is depicted in Figure I We believe that each stage has a particular objective; that whether or not those objectives are met is judged by the application of certain criteria which are different for each stage; and that each stage bears a particular relation to the change process.

Let me make a few explanatory comments about the figure.

1. Research is a process intended to advance knowledge. It may provide the basis for an invention but need not be conducted with that end in view. The appropriate criteria for judging research are internal validity and external validity. It need not be of any particular import to the researcher whether or not his findings have any "practical" application, and to expect him to conduct research with the intent of producing applications is to impose an irappropriate expectation for his work.

2. Development is a process which formulates a solution to an action problem (invention) and which packages this solution or invention

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[5]A more complete explication of this paradigm is contained in our paper, "An Examination of Potential Change Roles in Education" Symposium on Innovation in Planning School Curricula, National Education Association. Center for the Study of Instruction, Airlie House, Virginia, October, 1965.

FIGURE I

A CLASSIFICATION SCHEMA OF PROCESSES RELATED TO AND NECESSARY FOR CHANGE IN EDUCATION

	RESEARCH	DEVELOPMENT		DIFFUSION		ADOPTION		
		INVENTION	DESIGN	DISSEMINATION	DEMONSTRATION	TRIAL	INSTALLATION	INSTITUTIONALIZATION
OBJECTIVE	To advance knowledge	To formulate a new solution to an operating problem or to a class of operating problems, i.e., to innovate	To order and to systematize the components of the invented solution; to construct an innovation package for institutional use, i.e., to engineer	To create widespread awareness of the invention among practitioners, i.e., to inform	To afford an opportunity to examine and assess operating qualities of the invention, i.e., to build conviction	To build familiarity with the invention and provide a basis for assessing the quality, value, fit, and utility of the invention in a particular institution, i.e., to test	To fit the characteristics of the invention to the characteristics of the adopting institution, i.e., to operationalize	To assimilate the invention as an integral and accepted component of the system, i.e., to establish
CRITERIA	Validity (internal and external)	Face Validity (appropriateness) --- Estimated Viability --- Impact (relative contribution)	Institutional Feasibility --- Generalizability --- Performance	Intelligibility --- Fidelity --- Pervasiveness --- Impact (extent to which it affects key targets)	Credibility --- Convenience --- Evidential Assessment	Adaptability --- Feasibility --- Action	Effectiveness --- Efficiency ---	Continuity --- Valuation --- Support
RELATION TO CHANGE	Provides basis for invention	Produces the invention	Engineers and packages the invention	Informs about the invention	Builds conviction about the invention	Tries out the invention in the context of a particular situation	Operationalizes the invention for use in a specific institution	Establishes the invention as a part of an ongoing program; converts it to a "non-innovation"

for institutional use (design). Development, not research, is thus the process that actually produces and makes available an invention. Criteria appropriate to invention relate to judgments concerning its apparent validity for solving the problem it was intended to solve, its estimated viability in the real situation, and its probable impact, i.e., the relative contribution it might make to the solution of the problem. Criteria appropriate to design include feasibility within an ongoing institution, generalizability in the sense of utility in a wide variety of situations, and performance, i.e., how well does it get the job done? Development may or may not be based upon research; it may be desirable to so base it, but perhaps this approach may not be practical because appropriate research is lacking or because available research is ambiguous or incomplete. Such lacks exposed during the development phase are of course useful in identifying further needed research

3. Diffusion is seen as a process for informing persons about the existence of an invention (dissemination) and for building conviction about its utility on the basis of appropriate professional evidence (demonstration) Criteria suitable to dissemination include intelligibility (is the information clear?) fidelity (does the information present a valid picture?) pervasiveness (does the information reach all of the intended audiences?), and impact (does the information affect key targets appropriately?) Demonstrations, to be effective, must be credible, convenient, and provide opportunity for professional assessment of evidence, both positive and negative, concerning the utility of the invention being demonstrated. This criterion of evidential assessment is crucial for the aim of demonstration is not to huckster a particular invention but to open a further alternative for professional consideration.

4. Adoption is a process for trying out, fitting to, and establishing an invention as part of an ongoing program. A trial period is concerned with testing the invention in the particular setting in which it may eventually be installed; criteria appropriate to it are the adaptability, the feasibility, and the action of the invention in that setting. An installation period is concerned with fitting the characteristics of the invention to the characteristics of the adopting institution, a complex process that may require extensive redesign, personnel retraining, etc.; criteria appropriate to it include the conventional administrative criteria of effectiveness and efficiency. An institutionalization period is required to assimilate the invention totally, i.e., to convert it into a "non-innovation"; criteria appropriate to it include continuity, the degree to which the invention is valued, and the support given to it by the local setting.

The Selection of a Methodological Strategy

Given the preceding formulation of the change process, we are now in a position to make a choice between experimental and aexperimental strategies for studying educational change. We may begin by noting that this formulation opens two broad areas of inquiry to us, which I shall term change research and evaluation. Change research is concerned with the entire process, the entire content of Figure 1, so to speak. It may investigate the necessity and sufficiency of the formulation, of the contribution made by each phase, of the methods and agencies that interrelate and articulate the phases and the means used in carrying them out; in short, the whole gamut of inquiry as described in Bhola's paper. Evaluation, on the other hand, is concerned with single phases; it asks whether, in relation to any particular innovation, the objectives have been met; whether, for example, a particular design is generalizable and performs well,

or whether a particular demonstration is credible and based upon evidential assessment.

Which inquiry strategy seems, at this time and under present conditions, best equipped to deal with change research and evaluation, when seen in terms of the conditions outlined in Table 1? Let us consider the two areas of inquiry separately.

1. Change research. Most existing change research has been carried out in relation to real situations: adoption of hybrid seed corn by the farmer, spread of a particular drug in the medical profession, and the like. Broad classes of variables such as change agents, change mechanisms, characteristics of innovations, characteristics of early adopters, etc., have been identified and used. But it is clear, particularly in view of Bhola's excellent summary, that change research is in its infancy; no generalized systems of variables or theories have emerged. If the particular virtue of the experimental approach is that it permits the investigator to bear down hard on a few selected variables known to have high relevance, the utility of the method is questionable in this area where the general level of sophistication is so low. I believe it is fair to describe the situation in relation to change research as follows, using the terms of Table 1:

Intent of the Investigator: Possibilities, but with practical considerations having forced the examination of actualities.

Setting: Nature.

Level of Control: Efforts made at control but with a great deal of confounding evident.

Scope: Molar, although becoming more molecular as sophistication increases.

Number of Variables: Large, although not gross.

Treatments: Variable, although in some areas stability is beginning to emerge.

Context: Still quite relevant.

Under these circumstances, the power of the laboratory approach must be considerably diminished. It is my conclusion that aexperimental methods are now most appropriate to change research although experimental methods will be increasingly employed and should be utilized as interest of investigators indicates their use and conditions make their use possible.

2. Evaluation. Evaluation is a fairly well developed science. It is no longer viewed simply as a means whereby certain baseline data are compared to performance data in order to ascertain overall performance; instead, it has become increasingly useful as a tool for process control and as a steering mechanism to make adjustments and refinements long before the final performance data are in. The systems concept has gained sharp ascendancy, and evaluations are related very directly to the particular situations in which they are carried out. In the terms of Table 1, the situation with regard to evaluation may be described as follows, in my judgment:

Intent of the Investigator: Actualities.

Setting: Nature.

Level of Control: Invited interference.

Scope: Molar.

Number of Variables: Gross.

Treatments: Variable.

Context: Highly relevant.

In short, the evaluation process fits exactly the conditions described for aexperimental inquiry. While in the case of change research we might wish to argue that increased sophistication will make possible more experimental inquiries over time, it seems unlikely that the interest of evaluators

will tend toward experimentation. This is not to deny that experimental techniques have no place here, but simply that they are not the major tool. So for example, while an automobile manufacturer intending to develop a new carburetor for use on his product may subject prototype carburetors to laboratory experimentation to make determinations about design specifications, his major interest is in whether that carburetor will work on real cars in real situations. Similarly, components of an educational invention may, particularly in the design stage, profit from study under laboratory conditions, but the final and most meaningful evaluation must be made in the field with intentions and under conditions that make the aexperimental mode most meaningful.

It is therefore my considered judgment that both in the case of change research and evaluation, the aexperimental approach is at present most meaningful, and I urge it upon you as the preferred strategy, at least at this time. Since the term "aexperimental inquiry" seems somewhat pedantic for everyday use, I suggest that we refer to this general approach as a "field study" or "field investigation," in view of the fact that this strategy deals primarily with actualities in natural contexts. I will use the term this way throughout the remainder of this paper.

Some Illustrative Tactics for Field Studies

An immediate and somewhat disquieting consequence of my recommendation that we adopt the field study approach is that the methods and techniques that have been developed for experimental inquiry are inappropriate. One may well ask whether the recommendation is in fact feasible and viable (to use my own criteria), in view of this fact. I shall therefore devote

the remainder of this paper to a description of certain techniques which are illustrative of how the strategy may be pursued, we may think of these illustrations as tactics designed to carry out the strategy.

The suggestions that I will make must be understood against the background which I sketched earlier regarding the nature of field studies, and particularly, their unique characteristics. I take it for granted that we shall, in every field study, make every effort to collect a baseline of data about the situation, because the contextual nature of field studies makes this necessary if reasonable interpretations of data are to be possible. I shall further take it for granted that the methodologies must be largely non-interventionist, to satisfy the requirements of aexperimental inquiry. Finally, I shall assume that field studies will always involve collection of data on a large number of variables, recognizing the molar, multivariate nature of such inquiry.

I shall not spend any time discussing the fact that field study methodologies are already available in a number of substantive areas. In his methodological survey Bhola repeatedly mentions the research tradition in such areas as sociology, anthropology, psychology, psychoanalysis, economics, and communications. Obviously, each of these traditions will have something to contribute, and we can profit by studying their methods. I shall refrain from reviewing these standard approaches since they are generally available in the literature. Instead, I will get some of my more woolly ideas out in the hope that you will demolish them, and so get them out of the way once and for all, or help me to refine and enlarge them. The suggestions follow:

1. <u>Field studies must be approached programmatically because of their complexity and molarity</u>. The field investigator cannot observe everything,

how is he to know what is relevant? I submit that a systematic development of the objectives of the inquiry adequately placed within an appropriate theoretical framework is the only defensible approach except in the most simple cases.

What do I mean by a programmatic approach? I believe I can best make this clear with some observations on the nature of inquiry objectives.[6]

Now every objective which can be stated for an inquiry consists of (1) an action aspect and (2) a content aspect. Thus, for example, an evaluation study might pose the objective, "To determine the effect of PSSC materials on science achievement." The action aspect is represented by the phrase, "to determine," while the content aspect is the "effect of PSSC materials on science achievement."

Any given study is likely to have several objectives. It is possible to display the objectives in tabular form if each objective is first broken into its action and content aspects, and a two-dimensional table is then formed whose rows are defined by the action aspects and whose columns are defined by the content aspects.

Clearly the content aspects of a study depend on the substantive field which is being investigated, in my example, PSSC materials. But is it also true that the action aspects will depend upon the particular substantive area? If so, we have gained nothing by the tabular representation. But suppose that a set of terms could be developed about which

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[6]The discussion which follows is drawn from the paper, "Types of Educational Research," by Egon G. Guba and David L. Clark, Chapter IV in William J. Ellena (Ed.), Research and the School Administrator, Washington, D.C.: American Association of School Administrators, in press.

we might assert with some confidence that all possible research actions are represented therein? If so, one might project for any given substantive area all possible inquiry objectives, by linking the general classification system for action aspects with particular classification headings appropriate to the substantive field which is under investigation.

Unfortunately, such a generic classification of action objectives cannot be directly generated, but there does appear to be an intuitive solution to this problem which can subsequently be tested empirically. The four verbs (1) depict, (2) relate, (3) conceptualize, and (4) test, appear to account for all action aspects which might be postulated for any study.

This classification of action aspects was derived intuitively from the following reasoning An investigator dealing with a hitherto uncharted area is at a considerable disadvantage in studying that area. He has no preconstructed framework to guide his inquiry; about all he can do is make random observations, recording what he sees. Much of what he sees he will be able to depict in qualitative form only, but in some cases he will be able to make rudimentary measurements, counts, or ratings. Such operations can be designated with the generic term depict, and this depiction can occur in two ways, qualitative and quantitative The term describe can be used to indicate qualitative depiction and the term estimate to indicate quantitative depiction

An area well depicted is subject to a different strategy of inquiry Entities within the area may be related to one another or to entities outside the area. Again such relating may take quantitative or qualitative form. The generic term relate will be used to indicate this operation.

and the terms compare and correlate to indicate the qualitative and quantitative aspects of this operation.

Once relationships are established, it is possible to develop concepts about an area. Entities and relationships may be studied to determine groupings or classes Out ot these basic classes of entities and relationships may emerge theoretical conceptions, systems, paradigms, or models. The generic term conceptualize is used to mean the general process of attaching meaning to entities and relationships The term analyze will indicate the abstracting or categorizing aspects, while the term synthesize will indicate the developing, constructing, or theorizing aspects

Finally, given taxonomies or theories, we may return to the phenomenological levels of depicting and relating to test The operations or processes involved in testing the conceptual formulations may be indistinguishable from the operations or processes involved in depicting or relating, but it is clear that their purpose is vastly different.

The categories of the action taxonomy here proposed are summarized in Table 2 which in addition to the major and minor terms already mentioned contains a series of synonyms intended to help grasp the meaning intended at each level.

The proposed system outlined in Table 2 appears to have a number of advantages for programmatic planning:

1. The taxonomy when applied to an area of inquiry circumscribes the possible objectives which may be undertaken

TABLE 2

GENERIC CLASSIFICATION OF ACTION ASPECTS OF INQUIRY OBJECTIVES

Major Terms	Minor Terms	Synonyms
Depict	Describe	identify, define, distinquish, determine, limit
	Estimate	appraise, rate, count, rank, measure, standardize, norm
Relate	Compare	liken, contrast, collate, match
	Correlate	connect, associate
Conceptualize	Analyze	examine, categorize, abstract, reduce
	Synthesize	prepare, develop, construct, systematize, compose, assemble
Test	———	evaluate, confirm, resolve, demonstrate, substantiate, verify

2. The investigator is forced to assume a logical framework or theory to help him develop a provisional set of content columns, which he can improve as his sophistication increases

3. The investigator must make a conscious choice of the particular objectives which he will follow on the basis of explicit criteria relating to the objectives.

I would suggest that this approach will permit an investigator to outline in some detail just what it is he will do, within what theoretical or logical framework he will do it, and in what order.

2. Field studies must be conducted within an explicit theoretical or logical framework. It is obvious that the programmatic approach proposed in the previous section will not work unless appropriate labels, drawn from some theory or logical framework, can be applied to the columns of the objectives matrix. Where does this theory come from? Now I would not like my suggestion to immobilize all field studies while appropriate theories are being developed; we must obviously start from where we are. Even the most simple categories will do at first, for it is obvious that the process implied by Table 2 is cyclical in nature. However incomplete, naive, or even wrong the theoretical categories may be at the start, the processes of depicting, relating, conceptualizing, and testing will lead to improvements and refinements; these improved and refined formulations can then be used to generate new column headings which make possible more sophisticated and refined study.

A simple answer then to the question of where the theory will come from is to reply, from where we now are. For example, the headings from the Clark-Guba change paradigm in Figure 1 give one kind of start. The list of common generic variables that seem to be widely found in the change literature, e.g., change agents, change mechanisms, target characteristics, innovation characteristics, etc., is another. Chin's taxonomy of innovation types, i.e., substitution, alteration, perturbation and variation, restructuring, and value orientation change, is another.[7] Many other examples could be cited, most already noted in Bhola's summary of research.

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[7]Robert Chin, "Models and Ideas about Changing," paper presented at the Symposium on "Identifying Techniques and Principles for Gaining Acceptance of Research Results of Use of New Media in Education," Lincoln University of Nebraska, November, 1963

A more sophisticated answer to the question of where the theory is to come from is to suggest that it be systematically generated. I would refer you here to the work presently being conducted at The Ohio State University by my colleagues, George and Elizabeth Maccia, who are pioneering in what they call the retroductive generation of educational theory through the use of theory models from other fields. Briefly, the process involves the identification of an existing and productive theory in another field e.g., graph theory from mathematics or general systems theory, and in effect translating this theory into educational variables and processes to yield testable hypotheses about education. We may say, for example, that the processes of learning are analagous to the processes of visual perception, and since we know a good deal about visual perception, the laws, theories, and hypotheses of visual perception may, after proper "translation," serve, subject to further empirical test, as laws, theories, and hypotheses of learning. I would recommend a thorough study of the Maccias' work to any serious student of the change process. But let me emphasize that I do not believe that the existence of highly sophisticated theory is a necessary precondition to beginning work. It is rather a goal toward which we may work.

3 <u>Data collection in field studies is characterized by a unique relationship between the investigator and the phenomenological field</u>

This statement represents, in my judgment, the most important single tactic that I can describe in relation to field studies. Perhaps the best way that I can explain what I mean is to contrast the behavior of the typical laboratory investigator with that of the typical field investigator. The laboratory investigator is intent on manipulating

relatively few variables, with rigorous controls established for all others. The unwanted character of these other variables is indicated by the fact that they are termed "confounding" variables, and I am sure that the laboratory investigator often tends to think of them as "confounded" variables. At any rate, he sets an experimental design to control them, and the design is also selected to yield a particular information estimate that the investigator happens to be interested in. When everything is set, the laboratory investigator figuratively presses the "Go" button, and then stands back to let the experiment run. At the end he collects his already categorized data and analyzes them according to the design he has had in mind all along. If the confounding variables got a bit out of hand he curses silently, vows to build in more rigor next time, and reports his findings in ways designed to diminish the importance of his obviously inflated error estimate.

The field investigator is not, similarly, at the mercy of his data. Changes in conditions do not alarm him; indeed, he expects them, and fully intends to capitalize on them. Whenever changes occur he is there to note them, to make whatever shrewd guess he can about why they occurred, to estimate as carefully as he can their effect, and to establish a new base line, as it were, for subsequent data collection. He rolls with the punch, he is on top of things, he is not afraid to work with the data because he need not fear that to do so will destroy the careful balance of his experimental controls.

Let me give two examples of how the field investigator can accomplish this dynamic role. My first example relates to a project which Dr. Sidney Eboch and I have currently under way, and which is designed to determine

the effects on a school of the maximum availability of films and filmstrips. Four schools throughout the country have been provided with a wide variety of films and filmstrips--something over 500 films and 1000 filmstrips--together with the necessary projection devices, room darkening screens, etc. Our purpose is to gather certain data on the process of adjusting to this bonanza and to determining the effects of the program on student achievement, teacher curricular adjustments, and a wide variety of other variables.

Now we know from previous research on media that we can expect a particular utilization curve to manifest itself if we plot film use, say, against time. The typical form of the curve is shown in Figure 2. At first, we get a sharp rise in film use as compared to the base rate typical of the school before the project was begun. This rapid acceleration continues for some period of time, then suddenly, we reach a point of inflection, A, at which the curve begins a sharp deceleration. After a further time, at point B, the curve begins to level off and to asymptote to a final normal use rate.

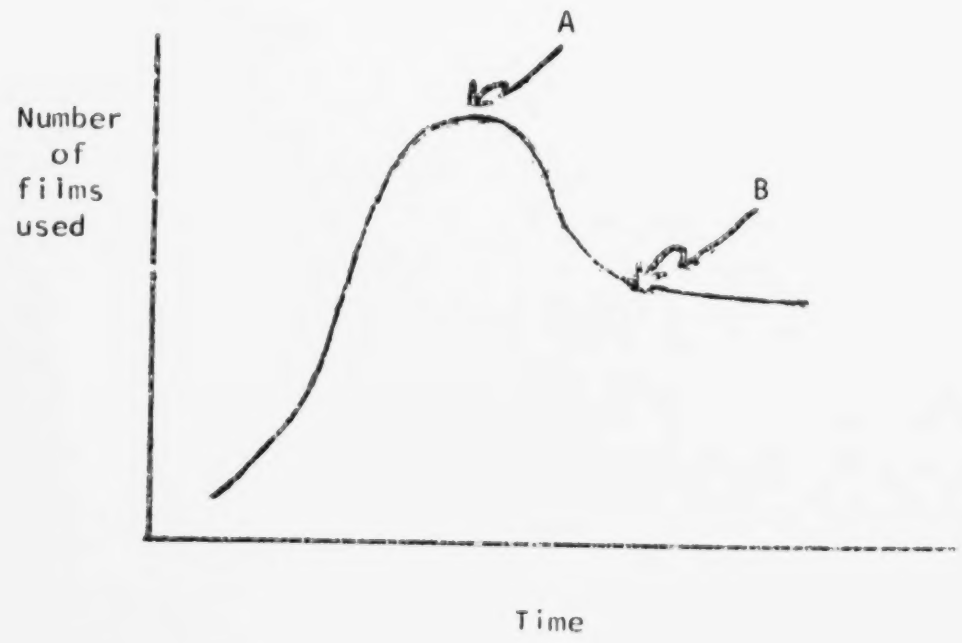

Figure 2 - Curve of Film Use Against Time

If we have been plotting these data systematically we can become aware very quickly when points A and B put in an appearance. Now is the time, the field investigator cries, and he is off with all of his observational and interview techniques to find out what is going on. What are people thinking about? Why are they suddenly surfeited at point A? What is the nature of the routine into which they settle at point B?

Clearly this technique of continuous time analysis of data for the sake of exposing crucial events and decision points can have great utility. The investigator may not know what to expect; he may not know when to expect it; he surely has not arranged it through experimental treatment definition; he has not controlled any of the possible sources of interference. Yet he can identify a crucial phenomenon and pursue it with whatever techniques seem appropriate.

I should also like to point out that, although in my example I began with a certain preknowledge of the form of the curve that I might expect, this preknowledge is not a necessary condition for the application of the technique Plotting use data against time in ignorance of typical use curves would still have produced the curve, and the investigator could still have made interpretations that crucial events were occurring.

My second example is concerned with what is sometimes called linkage analysis. How, for example, does a creative use of films in classrooms that may have been invented by a particular teacher spread to other teachers? Where did teacher X hear about it? How did she learn

enough about it to be able to utilize it herself? What is the chain by which the innovation spreads from its inventor to all of the other teachers in a building, and perhaps to teachers elsewhere as well?

Here a technique of retrogressive interviewing may be in order. We may find that the "chain" along which the innovation spreads corresponds, say, to the sociogram of informal social contacts among teachers. Again, the important point to note is that the investigator does not wait for the data to come to him; he goes after them wherever they may be found. In contrast to the relatively passive laboratory investigator who pushes the "Go" button and then sits back waiting for things to develop, the field investigator is constantly on the go himself, relating to the phenomena, fitting, shaping, matching until insights emerge and the basic patterns are nailed down.

4. Because of the probabalistic nature of field data, and the impressionistic way that these are gathered, constant replication and recycling are necessary to build confidence in conclusions. This tactic might be called the tactic of accumulative evidence. Now we all know that the flip of a coin is a singularly unreliable event in that it is entirely a random matter whether a head or tail comes up. But suppose that we have tossed the coin twenty times and find on each occasion that a head turns up. Now such an outcome is, of course, possible under random circumstances, if one tosses a coin often enough, eventually a run will occur in which twenty successive tosses come up heads. Yet this event is so rare that if you were to toss a coin with me right now and have a run of twenty heads, I would be strongly inclined to believe that the coin was biased in some way or that you were

manipulating it in some way while you were tossing it. If you should increase that run to twenty-five or thirty straight heads, I should be almost certain of that conclusion.

Many of the methodological problems in the social sciences have been foisted upon us by an almost slavish emulation of the methods of the physical sciences. but one of those methods, which we for some strange reason have decided not to adopt, is that of replication. No principle in the physical sciences was ever conclusively established by the critical experiment of a single investigator. So for example, when Michelson published his classic paper concerning the precise determination of the speed of light, many other physicists immediately set up similar apparatus and replicated or repeated Michelson's experiment for verification. It was not considered de classe by other physicists to replicate the experiment and to publish it and indeed the physics literature of the time contains reports of many such replications. In the social sciences, it seems to militate against the reputation of an investigator to repeat the work of another, but it is obviously through such continuous repetition and recycling that complete confidence in the validity of a result is built up. This must be especially true in an area such as field studies in which the data which result from any particular single study are likely to be more unreliable than we would really like. The method of accumulative evidence is one that ought therefore, to appeal especially well to us.

In applying this tactic we may also borrow a useful notion from Campbell and Stanley, who make the following point.

> . . . the more numerous and independent the ways in which the . . . effect is demonstrated, the less numerous and less plausible and singular rival invalidating hypothesis becomes. The appeal is to parsimony. The "validity" of the experiment becomes one of the relative credibility of rival theories: the theory that X has an effect versus the theories of causation involving the uncontrolled factors. If several sets of differences can all be explained by the single hypothesis that X has an effect, while several separate uncontrolled-variable effects must be hypothesized, a different one for each observed difference, then the effect of X becomes the most tenable.[8]

Campbell and Stanley's observation was of course intended to apply to experimental situations in which quasi-designs were required because of the experimenter's inability to meet all necessary control requirements but are nonetheless true also for field studies. To the extent that field studies can be conducted in overlapping 'styles' (I intend to use the term "styles" here as roughly analagous to the experimental term "designs") the effects of particular variables can at least be probed, even if not definitively established. If styles, like quasi-designs, are selected so that the weaknesses of one are matched by the strengths of another, reasonable inferences can be made.

5. At times, the major point of interest in a field study may be analagous to a "treatment" in the classic design sense although it may not have been introduced by the investigator and hence is not an intervention in the usual sense. Under such circumstances, quasi-designs are useful and may be applied. Quasi-designs of the types proposed by Campbell and Stanley are intended to serve as substitutes for true designs in situations where all relevant variables are not under the experimenter's control. That is, these designs were originally intended for use in connection with experimental or laboratory inquiry, and each of these

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[8]Donald T. Campbell and Julian C. Stanley, "Experimental and Quasi-Experimental Designs for Research on Teaching," Chapter 5 in N. L. Gage (Ed.), Handbook of Research on Teaching. Chicago: Rand McNally and Co., 1963.

designs contains an X factor or treatment condition imposed by the investigator. At times, however, the X may not be imposed from the outside but may naturally emerge in the situation, as for example, when certain administrative constraints may be placed upon the use of films and filmstrips by teachers in the project that I cited earlier. These constraints in effect form a treatment condition that may materially affect the uses that teachers make of the available media, and the field investigator may wish to determine the effect of these constraints. Stanley has suggested that under these circumstances the design which may be diagramed (using the notation of the Campbell-Stanley chapter already cited) as follows has utility.[9]

$$O_1 \quad O_2 \quad \vdots \quad O_3 \quad X \quad O_4$$

O_1 and O_2 refer to observations made at a previous similar time interval (e.g. the previous school year), and while O_3 and O_4 are observations made currently, with X, the emergent variable of interest simulating a treatment variable being juxtaposed. The dotted horizontal line reminds us that the two groups on which the observations are made may not necessarily be parallel or equivalent groups. The effect of X is estimated by comparing $(O_4 - O_3)$ with $(O_2 - O_1)$. This design is subject to a number of threats to validity, which I shall not review here but which are discussed in some detail in the Campbell-Stanley chapter.[10]

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[9] Julian C. Stanley, "Quasi-Experimentation in Educational Settings." Conference on Novel Strategies and Tactics for Field Studies involving New Educational Media, The Ohio State University, Columbus, May, 1965.

[10] Campbell and Stanley, op cit., 178

The major threat to this design, history, that is random time related effects, can be considerably reduced, according to Stanley if it is possible to add several control groups, one within and one outside the experimental school. The design may then be diagramed as follows.[11]

$$\begin{array}{lll} -\underline{O_1}-\ -\ -\ -\underline{O_2}-\ -\ -\underline{O_3}-\ -\ -\ -\ -\ -\underline{O_4}- & \text{Experimental} \\ \qquad\qquad\qquad\qquad\quad O_5 \qquad X \qquad\quad O_6 & \text{School} \\ -\bar{O}_7-\ -\ -\ -\bar{O}_8-\ -\ -\bar{O}_9-\ -\ -\ -\ -\ -\bar{O}_{10}- & \text{Control School} \end{array}$$

$(O_2 - O_1) - (O_4 - O_3)$ affords an estimate of the history effect within the school; this estimate may be compared with that derived from the comparison $(O_8 - O_7) - (O_{10} - O_9)$ the history effect in a control school. O_3 O_5, and O_9 should be similar as possible to afford group comparability The effect of $\underline{X}$ is of course determined as before, except that multiple comparisons are now possible $(O_6 - O_5)$ may be compared with $(O_2 - O_1)$, with $(O_4 - O_3)$, with $(O_8 - O_7)$ and with $(O_{10} - O_9)$.

6. The fact that experimental control is not possible or even necessarily desirable in field studies does not mean that the investigator is forced to use just any samples or situations. Purposeful selection is a powerful tool in focusing upon variables of interest to the investigator We have now repeatedly made the point that field studies cannot be conducted in conformity with the conditions of control required by classic experimental approaches Indeed the requirements

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[11] Stanley, op.cit.

of classic design for sampling (which you may recall requires both random selection of subjects and random assignment to treatments) are also typically not met. But the fact that these conditions are not met should not cause us to throw up our hands and simply use any situation or any sample which happens to be convenient. Rather we have the option of selecting situations and samples with a view to achieving maximum differentiations of the factors in which we happen to be interested. Let us suppose, for example, that we have developed a new course in biology which we wish to try out. We are well aware of the fact let us say, that different teachers will regard this biology package with different attitudes. Some teachers whose training in biology may be rather poor may very well welcome the package as a way to assure themselves that they will conduct their classes at least at a minimum level of quality Other teachers may feel that the use of such course materials prevents them from exercising their own ingenuity and initiative with regard to teaching the course since they must slavishly follow the outline as it has been developed

Now we could try out this biology course in a series of convenient schools, and we might quite by accident, uncover teachers who hold these different attitudes. It might then be possible to make some statement about the interaction of these attitudes with the use of the course materials. On the other hand, it is equally likely that if we select a convenience sample, groups sharply differentiated on this attitude will not be identified and information with regard to the interaction of attitude and utility will be unavailable. We can insure the availability of such information by the way in which we select our sample, however. Obviously, we could select as one of the schools in which the materials are to be

tried, an upper socio-economic level school which has the resources to hire excellent, well-experienced teachers who are likely to set high store upon their own ingenuity and creativeness; and select as another school, one which has a faculty that can be identified as relatively unimaginative and uncreative in its approach to teaching. By such judicious selection of the sample, we can get at least a partial reading on the relationship of attitudes to utility which might not otherwise be available. Obviously, I am not suggesting that such a design will permit absolute cause and effect statements; I am suggesting that under the typical conditions of field studies, such an approach is more likely to yield useful information than is an approach which does not take advantage of naturally occurring differentiations.

7. Special techniques of analysis and interpretation need to be developed which are especially suited to the data produced by field studies. Many of these special techniques may be deduced by analogy to similar techniques in related areas. Certain other areas of endeavor, while not faced with methodological problems identical to those which face the field investigator, have nevertheless developed useful ways of handling the same kinds of data which he usually collects. So for example, much of the data that the field investigator obtains are based upon the testimony of persons: teachers, pupils, administrators, parents, and others. Such testimony is usually dismissed by classic experimentalists as worthless because, as they say, it is highly subjective, influenced by Hawthorne Effect, distorted by the participants into a self-fulfilling prophecy, and the like. Yet, there exists an analogous area in the courts, whose operations and whose judgments are very often based upon testimony. Over the years well developed principles of evaluating testimony have emerged; thus, we are all familiar

with the fact that hearsay evidence is inadmissible, that eyewitness testimony is preferable to circumstantial evidence, that expert witnesses may have their opinions regarded by the court as literal fact, and so forth Is it possible that through an examination of the "rules of the game" established for testimony in the courts we may deduce some methodological principles which will have utility for us in weighing the testimony of participants in field studies?

Another form in which the data of field studies often comes is that of judgments made in observational situations. Other analogous areas exist in which judgments based upon observation have been developed into a fine art. So for example, artists have learned to make judgments about paintings, musicians have learned to make judgments about "the innate talent" of other musicians whom they hear perform; schools of speech have learned to develop debate judges whose opinions regarding the relative quality of various debate teams are highly reliable and valid; athletic departments have learned to make judgments concerning athletic performance; schools of agriculture have learned to train stock-judging teams whose judgments about the quality and carcass weight of an animal examined on the hoof are remarkably reliable Is there something to be learned by studying the methods used in these analogous areas? A third analogous area is that of the developmental case study Particularly in recent years the literature of a number of fields, particularly the military, have contained reports concerning the development of some particular project, as for example, the B-70 bomber. Intricate case studies of various phases and aspects of the development are pieced together to give a very coherent picture of the process by which the final end product was achieved. Again, we see the utility of such detailed case studies in relation to educational field studies.

There are obviously other analogous areas which could be mentioned but my purpose here has been to be illustrative rather than exhaustive. Clearly, these analogous areas should be extensively studied for clues for the development of field study methodology.

8. A most important tactic in planning field studies is to lean more heavily upon logical inference than upon statistical inference. It has been common in previous field studies to set only the most simple minded hypotheses for test but to depend upon an elaborate statistical approach in reaching conclusions. We have now repeatedly made the point that field studies cannot meet the assumptions of classical systems and design, nor indeed, do we want them to. To rely heavily upon the statistical treatment thus, seems to be the height of folly. This folly is well documented by the fact that so many of our field studies have resulted in inconclusive findings. The most readily available example that I can think of is the large number of field studies that have been conducted in relation to the effectiveness of educational television. Repeatedly we have tested the weakest possible hypotheses, to wit, can television teach as well as "conventional methods?" Over and over, our statistics have forced us to the same conclusion: there is no significant difference between teaching by television and teaching by conventional methods. Indeed, researchers in this area have formed themselves into the N.S.D. Club, i.e., the No Significant Difference Club, because of the frequency with which this finding occurs. I would suggest that the fault here lies not with any innate deficiency in the field study approach but rather in the weak hypothesis which the field study is intended to investigate. We have surely not exhausted the logical processes of which our minds are capable in producing hypotheses at this level.

What seems to me to be a more rational approach is to imagine every alternative outcome that might result from the particular field test that we have in mind, and then take steps to see to it that these possible alternatives are systematically ruled out if they are invalid but retained if they are valid. This approach has been clarified by Platt in an interesting recent article which I urgently recommend as reading to anyone interested in this area.[12]

Platt suggests a return to classic Baconian research and proposes the following steps.

1. Devising alternative hypotheses--as many as the investigator can imagine.

2. Devising crucial experiments with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses.

3. Carry out the experiment so as to get clean results.

4. Recycle the procedure, making sub-hypotheses or sequential hypotheses to refine the possibilities that remain.

Platt's advice will need to be adapted to field studies, of course, to take account of the fact that laboratory experiments are not possible within this rubric, but systematic and pointed observations are. We can certainly take a leaf from his book when he says

> Strong inference, and the logical tree it generates are to inductive reasoning what the syllogism is to deductive reasoning, in that it offers a regular method for reaching firm inductive conclusions one after the other as rapidly as possible.[13]

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[12] John R. Platt, "Strong Inference," Science, 146 (October 1964), 347-53.

[13] Ibid.

9. Another strategy which we can employ is to take as much advantage as possible of serendipities, or "natural breaks." To use again my example of a new course in biology, let us imagine that the schools in which these materials are to be used have agreed to a three-year field testing period. During that time many changes will occur in the school. Some of these will be related to the biology course and others will not. So for example, it is conceivable that during that three-year period there will be some turnover of teachers that will make it possible for us to contrast the attitudes and methods of teachers who have been using the material for some period of time with the attitudes and methods of teachers who are being exposed to it for the first time. It is unlikely in a typical field study, that we could exercise enough control over the situation to cause such a circumstance to occur by design, but there is, of course, no rule of the game that prevents us from taking advantage of this happy circumstance when it does occur. Other changes which occur naturally can similarly be taken advantage of.

10. A final suggestion that I might make is that we utilize the classic technique of analyzing pathologies to gain insights into natural situations. I should like to draw an analogy here to psychoanalysis as a technique for gaining insights into the dynamics of the human personality. Freud in his early days faced a problem similar to that of the field investigator in many ways. He could not control the objects of his study, he could not experimentally manipulate variables, he could not intervene, he had to take his subjects in a naturalistic setting. No one would argue that his patients were in any sense representative of the population at large in terms of personality or emotional variables

Yet analytic techniques directed at an understanding of the pathology exhibited by his patients proved to be the sword point by which Freud was able to pry open the oyster of the human personality. Had he asked what was right rather than what was wrong about his patients, his insights would have been much less incisive, I am sure.

I am suggesting that we take a similar tack in managing field studies. Things that go right are not likely to be very useful in understanding the dynamics of change, but things that go wrong may very well result in penetrating insights. Field studies that turn out "badly" because of situational or organizational pathology may in many ways be as important as those that turn out very well indeed. Our mistakes are as instructive as our successes, and deserve as careful an analysis.

Finale

I have taken you on a rambling tour of some as yet ill-formed ideas concerning the "new methodology" of field studies. This methodology is philosophically, intentionally, and technically vastly different from conventional experimentally methodology, as I have tried to show. It is not a poor imitation of research but an entirely different kind of inquiry.

Unfortunately, as is plainly evident from the unfocussed nature of my remarks, the "new methodology" is by no means systematically explicated. This task awaits the careful attention of methodologists more competent than I to give it rigorous form.

I hope, however, I have been able to give enough examples of forms which it might take to evoke in you confidence that these methods have promise. It is very clear to me that some new approaches must be developed and tried, if we are to meet the demands of the next decade adequately. If I have not convinced you of the utility of my particular suggestions, I trust that I have at least given you cause for concern regarding conventional strategies. If I have planted a festering doubt, more than half of my purpose will have been accomplished.